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## Wild Boar or Domestic Pigs? Response to Evin et al.

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### **Abstract**

We continue to contest the claim by Evin et al. that Rosenhof E24 and other Mesolithic pigs were domestic. E24's mixture of 'wild' and 'domestic' traits is best explained as indicating a behaviourally wild boar with some domestic ancestry. The fascinating complexities of this situation should not be downplayed in favour of a simplified, more newsworthy (but probably incorrect) conclusion.

### **Key words**

wild boar, domestic pig, Mesolithic, ancient DNA, Geometric Morphometrics, Rosenhof

We welcome Evin et al.'s comment on our critique of their recent paper (Krause-Kyora et al. 2013). With the clarifications and amendments made here, our respective interpretations of the interesting results presented in the Krause-Kyora paper do indeed seem to overlap. There remain, however, some significant outstanding differences on which we would like to elaborate here.

## **Domestication**

We concur with Evin et al. that domestication is a complex process and that a large and multi-faceted middle ground lies between a wild animal (or plant) and a full-fledged domesticate. We also agree that there is no single, easy way to cleanly delineate the point at which a wild animal has become a domesticate. Clearly a more profitable avenue for domestication studies is, as Evin et al. suggest, to move beyond making “one of two dichotomous status calls” and concentrate instead on exploring the ramifications of cases like the mixed ancestry of the Mesolithic pigs in the Krause-Kyora et al. study. This was, in fact, a major point of our original contribution: we maintained that the results of the Krause-Kyora study indicate only that Mesolithic hunters had access to pigs with some domestic ancestry; this had no bearing on whether Ertebølle foragers treated, or even recognized, these animals as domesticates.

We are happy to see that Evin et al. concede this point (at least to some extent) and disavow interpretations of the original Krause-Kyora article that claimed their results document the earliest example of pig rearing by Ertebølle foragers. But we feel we should point out that the authors of the original article bear more responsibility for this interpretation than is indicated here. This is not, as maintained by Evin et al., a simple case in which academics and, especially, the media have misinterpreted or over stated the conclusions of the original paper. While Krause-Kyora et al. were indeed careful not to claim that the Ertebølle tended domestic pigs, throughout the article they make statements that actively promote (a) the notion that Ertebølle foragers acquired domestic pigs, and (b) that this contributed both to the domestication of native European wild boar, and the eventual adoption of domestic pigs and other domesticates by foragers in the region. They use the unambiguous words ‘domestic’ and ‘domesticated’ in their title, three times in their abstract, and over a dozen times in their text, specifically to refer to Ertebølle pigs. To quote just one sentence (emphasis added):

... these *domestic* pigs ... represent not only the *first domestic* animals identified from Mesolithic sites in continental northern Europe, but also the *earliest domesticates* from the region—appearing some 500 years before the first reliable evidence for domestic cattle, sheep or goat (pg 5).

However, as Evin et al. agree, there is no evidence that Ertebølle foragers tended or managed these animals in a manner consistent with a domestic relationship or, indeed, that they treated these animals any differently than any other hunted boar. Even were the Mesolithic pigs studied in Krause-Kyora et al. to display a full suite of

morphological and genetic characteristics consistent with full fledged behavioural domesticates (small size, molars with a domestic molar shape, Near Eastern matrilineal haplotypes, and homozygosity in the *MC1R* allele), if they were not tended and managed in a way consistent with a domestic relationship, then it is hard to see how their consumption by Ertebølle hunters has any bearing on the process of domestication of local wild boar or the eventual adoption of agriculture by Ertebølle successors some 1000 years later. We already know that the Ertebølle were in contact with near-by farming communities and actively traded with them for a variety of material goods (see Rowley-Conwy in press). They were certainly aware that these farmers cultivated crops and husbanded animals. The acquisition of a domestic animal or two from their neighbours, either through trade or stealth, if it does not lead to the development of a domestic partnership between the animal and its keepers, says little about either local domestication or the assimilation of agricultural practices in the region.

But the pigs recovered from Mesolithic contexts documented in this study do not display this full suit of domestic traits. Instead they possess a mixture of wild and domestic genetic and morphological characteristics that, as we all agree, show that these animals had some domestic ancestry. This uneven and variable mix of traits makes it unlikely that these were indeed behaviourally domestic animals. Their large size alone precludes the possibility that they were, as claimed by Krause-Kyora et al., simply obtained through direct contact with Neolithic farming neighbours – the farmers simply did not have any domestic pigs that were so large. The more likely possibility is, as we argue, that these animals were the product of hundreds of years of introgression between feralized domestic stock into wild herds, and that they were taken from those herds by Ertebølle hunters who were entirely unaware of their domestic ancestry.

### **Why specimen E24 is not a domestic pig**

Despite accepting a number of our arguments, Evin et al. nevertheless advance three reasons “why specimen E24... is not a wild boar”. We find there is ample room to counter each of these arguments.

1) Coat Colour: Given the 500+ years in which loosely managed domestic pigs from Neolithic farmsteads likely escaped into nearby forests and interbred with native wild pigs, we see no reason why hybrid descendants of these animals might not be homozygous for the *MC1R* coat colour gene, even if possession of this trait puts the animal at a selective disadvantage in the wild. Rosenhof E24 was after all hunted and killed by people, perhaps because its spotted coat put it at a selective disadvantage. The continuous injection of these genes into the wild population by escaped domesticates would insure that these genes would not be weeded out.

2) Phylogeography: We are not clear why the presence of the Near Eastern Y1 haplotype is seen as a problem. We accept that it was brought into Europe in domestic pigs that descended from Near Eastern wild boar. Escaped domestic

females then carried it into the wild population. We discuss reasons why this haplotype does not occur today in European boars in our comment, and we reiterate here that since this is the first aDNA study of Mesolithic pigs, there is no precedent to say that this haplotype is unknown among pigs from Mesolithic contexts.

3) Molar size and shape: We do not contest the results of GMM; we see no problem in behaviourally wild boar carrying elements of a domestic molar shape, just as they carried the Y1 haplotype, and for the same reason.

What we contest is the claim made by Evin et al. (2013) that only 77.9-87.5% of animals can be correctly ascribed to wild or domestic using *traditional* biometry. As we pointed out, this claim is based on wild boar of varying sizes: small ones from Morocco, to large ones from Russia (Evin et al. 2013, supplementary table 1). Different wild boar populations are of very different sizes (Albarella, Dobney and Rowley-Conwy 2009). It is essential that individual populations form the metrical units of comparison; when this is the case, a much greater percentage can be determined. For example, in our fig. 2, seven teeth fall in the overlap zone while 102 do not, a success rate approaching 94% (see Rowley-Conwy, Albarella and Dobney 2012, 13-23 for discussion and further examples).

Adding measures of the shape of skeletal elements to the mix of approaches to documenting domestication may well prove a useful tool, especially if shapes distinctive of domesticates are consistent across regional variations in the size of animals. But we caution that, like many new techniques, closer scrutiny may well find that initial claims currently made about the application of GMM to complicated problems like domestication, commensalism, and dispersal may not be supported – or will at least need qualification. Most troubling is the lack of a clear understanding of the factors responsible for molar shape and how these factors are related to processes, like domestication, that molar shape is purported to measure. Demonstrating a clear and unequivocal relationship between the domestication relationship and a proposed marker of this relationship is an important prerequisite for any method used to document domestication – one that is too frequently ignored in the rush to promote new techniques for studying this complex phenomenon (see Zeder 2006).

Above all, we agree with Evin et al. that documenting domestication in the archaeological record requires the application of a variety of different tools. This means not ignoring, as they do here, a measure like size that has for many years been effectively used to distinguish introduced domesticates from native wild boar in Europe (see references from Rüttimeyer 1862 to Rowley-Conwy, Albarella and Dobney 2012). Instead a more profitable way forward would be to consider size as another variable that needs to be added to the mix in trying to sort out the complicated pathways animals take to domestication. Another future avenue of research would be to apply methods for reconstructing age and sex harvest profiles in pigs currently under development (Lemoine et al. 2014) to pig assemblages from the region as a means of assessing the evolving relationship between humans and

target animal species that lies at the centre of the domestication process (Zeder 2006).

Considering the mix of characteristics evidenced in the E24 specimen (Near Eastern matrilineal haplotype, homozygous spotted coat, and domestic molar shape, *and* its large size), we think we can more convincingly argue against the proposition that this was a domestic animal. As Evin et al. (fig. 1) show, first-generation crosses between wild and domestic animals will be of small (domestic) size. Rosenhof E24, however, is of large (wild) size (see our fig. 2), so its domestic ancestry is evidently more distant. The nearest known farming settlement is >150 km from Rosenhof (see our fig. 1). We do not know how quickly genes from domestic pigs that escaped from the farmers might take to spread so far through the wild boar population – perhaps several or many generations. By the time they appeared in E24, they are unlikely to have been relevant to the behaviour of that animal. In this instance we therefore believe we can suggest a clear status call: despite possessing some domestic ancestry, E24 was most likely behaviourally a wild boar, hunted by the inhabitants of Rosenhof like their other wild prey – perhaps made more vulnerable to human predation by its spotted coat, an example of the selection against homozygosity in this coat colour trait among wild boar in action.

## **Conclusion**

We find it curious that after the lengthy discussion of the perils of making either/or calls about the status of animals that show such a mix of characteristics, Evin et al. go on to declare Rosenhof E24, and the other mixed ancestry Mesolithic pigs in their study, unequivocal domestic animals. Moreover, despite admitting that their results shed no light on whether Ertebølle foragers tended these animals in a manner consistent with domestication, or even recognized them as possessing domestic ancestry, they still conclude that their study provides evidence of the earliest domesticates in the region.

The persistence of these claims speaks to an unfortunate tendency in so much of the literature on domestication to overstate results and push the simpler story line over the more nuanced and, to our mind, more interesting interpretation of complicated data. The prevalence of this approach in so many high profile publications on this topic is probably why both of us, individually, reacted so strongly to this article when it was first published. Our shared concern over this trend is most certainly why we came together to write our original comment, and this response to the Evin et al. reply.

In making these claims, the authors of this important piece of research bury a much more interesting interpretation of these data. We contend that a more defensible interpretation of the results of this study is that it provides evidence for the feralization of Neolithic domestic pigs, and an extensive degree of hybridization between these feralized animals and native wild boar – a remarkable demonstration of the value of the combined approaches to studying archaeological remains utilized

here. Instead of claiming firsts, the focus of this study should have been on the much more interesting question of why, despite the degree of apparently continuous, multi-directional gene flow between wild and domestic pigs, Ertebølle foragers nevertheless did not embrace the husbandry of domestic pigs (and the other agricultural practices) of their near neighbours. Ertebølle people consumed animals of mixed ancestry; but there is no evidence that they managed herds of domestic pigs, or indeed utilized any of the livestock or crop species that nearby farming communities relied upon so heavily. Even with extensive hybridization between wild and domestic pigs in this region, and the clear contact between Ertebølle foragers and Neolithic farmers, there must have been factors that led Ertebølle people to reject these new subsistence strategies, and all their attendant baggage, in favour of the highly successful broad spectrum foraging strategies that had sustained them for over 1000 years and continued to do so until 3900 cal BC. So rather than claiming to have found a catalyst that, after a 1000 years, led, through some unspecified way, to the adoption of agriculture, it would seem that the results of this remarkable study should have caused its authors to ask why the Ertebølle continued to resist this new technology and why the follow-on TRB people elected to embrace them.

This would have been the harder story to tell. But if we shy away from addressing these more difficult questions in favour of simpler, more newsworthy scenarios, we diminish the real accomplishments of a study like this, hardly helping our understanding of the European Neolithic or the forces that shaped the origins and dispersal of domesticates and agricultural economies. In so doing we sell short the increasing power of our discipline to grapple with difficult questions that lie at the heart of cultural evolution, and we perpetuate the public impression that archaeology is merely a source of “special interest” stories that round out an otherwise slow news day.

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